

## OUR BOOK SHELF

*British Manufacturing Industries.*—Edited by G. Phillips Bevan, F.G.S. "Jewellery," by George Wallis; "Gold Working," by Rev. C. Boutell; "Watches and Clocks," by F. J. Britten; "Musical Instruments," by E. F. Rimbault, LL.D.; "Cutlery," by F. Callis. (London: Stanford, 1876.)

THIS little volume (which is intended for popular reading) is comprised of several short essays, by different writers, upon the separate subjects indicated. Each essay contains a fairly good account of the history and general trade position of its subjects, but so far as their mechanical construction and the manufacturing operations involved therein are concerned, all are more or less disappointing. No doubt this is in great measure to be attributed to the nearly entire absence of diagrams, the essay on watches and clocks alone being illustrated, and that but scantily. Naturally some subjects suffer more than others. In jewellery, gold working, and cutlery the forms produced are familiar, the tools employed are simple, and what is the method of shaping and fitting together the various portions can easily be imagined. But with musical instruments and watches and clocks the case is different; people, *a priori*, are unacquainted with the apparatus or mechanism made use of, and a free reference to diagrams or figures becomes indispensable. In the essays upon jewellery and gold working, especially in the latter, their aspects and bearings as branches and developments of art are particularly dwelt upon. Cutlery, of course, is treated as an industry, so are watches and clocks. We are afraid the last-mentioned essay is not very carefully written, the writer, amongst other things, actually forgetting to tell us that there is any connection between the length of a pendulum and the time of its swing. And what he can be thinking of to describe Huyghens as a "French clock-maker of eminence," who "about 1650 showed great skill and ingenuity in arranging pendulums to clocks, so as to describe a cycloid," we do not know. The essay upon musical instruments (considering its not being illustrated) is much more intelligible than it might have been.

The book is neatly bound and printed, but will require considerable alteration and extension before it becomes what from its title we expected to find it.

*An Introduction to the Osteology of the Mammalia.* By Prof. W. H. Flower, F.R.S. 2nd Edition. (London: Macmillan and Co., 1876.)

PROF. FLOWER'S valuable "Osteology of the Mammalia" holds so high a position among scientific manuals that the appearance of a second edition requires but a passing notice from us. The author is himself so continually adding to our knowledge of the anatomy of the higher Vertebrata, at the same time keeping fully *au courant* with the investigations of both British and foreign zoologists, that there are several minor additions which he has had to make after an interval of six years, since the appearance of the volume originally. Amongst the most important of these, we notice the record of the conical form of the odontoid process of the axis vertebra in the Chevrotains (*Tragulina*), the introduction of a summary of Prof. Parker's study of the development of the skull of the pig, the account of the hyoid bones of the Ant-eater, of the large pectineal process in Phyllorhine Bats, and of the peculiarly ankylosed tarsus in the Muntjacs. In the first edition the typography and the printing of the woodcuts was too black throughout; in the new one this defect has been entirely removed, both the type and the figures being all that can be desired. There is a new outline diagram of special interest introduced to illustrate the mutual relations of the various elements of what may be termed the typical mammalian skull. This re-

places a plan drawn out for a similar purpose in which the names of the bones were distributed over a page in such a way as to indicate their relative positions. In the new diagram the employment of outlines to the bones renders the exact situation much more distinct and enables the commencing student to carry away with him a much more precise idea of the exact neighbourhood of each part of each bone than was possible from the older plan. We welcome with much pleasure this new edition of the "Osteology of the Mammalia."

## LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

## On the word "Force"

PROF. TAIT in his lecture on *Force* said that this word must be used in a certain definite sense and in this sense only. In order to claim Newton's authority for the one definite sense to which he would confine the word, he has to assume, not only that Newton translated *force* by *vis impressa*, but that he—an Englishman writing in Latin—used *vis insita*, *vis motrix*, &c., without any English equivalents. Until good evidence for these assumptions—improbable as they are on the face of them—is brought forward, Prof. Tait cannot claim the authority of Newton in his favour.

In the communication I made to NATURE (vol. xv. p. 8) I contended that the authority of Newton was *against* the restriction of the word to this one sense, on the assumption that the equivalent of Newton's word *vis* is *force*. To those who demur to this assumption I propose the questions: (1) Is it likely that Newton had in his mind no English equivalents for *vis insita*, *vis gravitatis*, *vis centrifuga*? (2) If *force* is not English for *vis*, what English word had Newton in his mind? Until some new light is thrown on these questions I maintain that Newton's authority is claimed for the restriction of *force* to the sense of *vis impressa* on, to say the least, insufficient grounds; and that the obvious interpretation of Newton's words is dead against it.

I have, I hope, in a previous communication done justice to Prof. Tait's zeal for definiteness and accuracy; and with him I feel what supreme virtues these are in a scientific man. But I contend that the wide sense of the word *force*—which I attribute to Newton—is not loose and inaccurate; it is simply general and comprehensive; each of the narrower uses, as in *vis impressa*, *vis insita*, is not more accurate but more special: these special senses are not inconsistent, though they are not identical; they are neither inconsistent with each other nor with the use of the word *force* in its widest sense. Some English mathematicians wish to have this valuable word all to themselves for a special technical sense; Newton claims no such monopoly, nor is it claimed by all foreign mathematicians, nor conceded by metaphysicians; nor is the claim to this monopoly likely to be conceded until a better title to it has been shown.

Cambridge, November 24

P. T. MAIN

## Peripatus N. Zealandiæ

In the November number of the *Annals and Magazine of Natural History* is a paper by Capt. Hutton on *Peripatus N. Zealandiæ*, in which the author comes to the astounding results that this species is hermaphrodite, and that its horny jaws are not foot jaws but homologous with those of annelids such as *Eunice*. If such were in reality the case much of my results concerning *Peripatus capensis* (*Phil. Trans. R. Soc.*, 1874, vol. clxiv. Part 2) would lose its value, and since I believe *Peripatus* to be a most important form, and a representative of the ancestral stock of all tracheates, in fact of the Protracheata of Prof. Haeckel, I hasten to write a few words in reply.

I obtained specimens of *Peripatus N. Zealandiæ* at Wellington from Mr. W. T. L. Travers, who has done so much for science in New Zealand, and who most kindly assisted me and my late colleague, R. von Willemoes-Suhm, in many ways, and who first pointed out *P. N. Zealandiæ* to Capt. Hutton also. I had further at least fifty specimens of *Peripatus* collected for me and brought to me alive. I examined these and made notes, but have been prevented by other work from publishing

them hitherto. *P. N. Zealandia* is not hermaphrodite. I examined several males, which differ in no essential points in their structure from those of *P. capensis*. Like those of *P. capensis*, they are less numerous than the females, and Capt. Hutton has been unlucky enough not to meet with any amongst the twenty specimens examined by him. The jaws of *P. N. Zealandia* are further, I believe, developed just as are those of *P. capensis*. At least I saw that the earliest stages corresponded, and recognised the first pair of members of the embryo in *P. N. Zealandia* in the stage in which they are not yet turned inwards to become foot jaws. I have prepared a more extended answer to Capt. Hutton's paper with an account of my own observations on *P. N. Zealandia* for the *Ann. and Mag. of Nat. Hist.*, but as this cannot probably be published immediately, I should be much obliged if you would insert this reply in NATURE.

H. N. MOSELEY,  
Naturalist to H.M.S. *Challenger*

### The Age of the Rocks of Charnwood Forest

IT is no doubt to be regretted that Mr. Woodward, misled by insufficient authority, should have introduced, in his excellent work on the geology of England and Wales, still further confusion into the maltreated old rocks of Charnwood Forest, but I doubt whether their age is quite so certain as Prof. Hull seems to think. I fully agree with him that there is not a particle of evidence for their Laurentian age, and that their syenites and hornblende granites cannot be correlated with the hornblende gneiss of the Malverns, but I must demur to his grouping them with the Cambrian rocks of the Longmynd or of Llanberis. The authority of Prof. Sedgwick is great, but it must be remembered that the term Cambrian with him included far more than in the nomenclature of the Geological Survey, and I am not aware that he ever committed himself to the Charnwood rocks being equivalent to his Lower Cambrians. Except a slight lithological resemblance of some Charnwood rocks to those of Harlech and Llanberis, and a still slighter to Longmynd rocks, there is really nothing in favour of this special correlation. One point, however, there is which may give some clue to their age, which does not seem to have been much noticed hitherto, probably because the facts have been strangely overlooked in the Geological Survey description of the district. It is that beds of coarse volcanic agglomerate and ash abound among the Charnwood series. Further, the resemblance of the rocks as a whole (when not unusually metamorphosed) is very close to the "green slate and porphyry series" (or Borrowdale rocks) of the Lake District. Compared with the Welsh rocks, they are far more like those of Cader-Idris than of Llanberis. With these there is scarce any lithological resemblance, but if I mixed my Charnwood collection with those from the other two localities, especially the former, I should have great difficulty in separating many specimens. It seems then to me far more likely that this great volcanic activity in the Charnwood district should have corresponded in time with that in the Lake District or with some part of that in Wales, than that it should have happened in the age of the Harlech, Llanberis, and Longmynd groups, where we have no evidence of any volcanic disturbance. The argument may be summed up thus, as it seems to me:—The Charnwood rocks are old, so are both the competing groups; they are unfossiliferous, so are both; they are cleaved, so are both; they contain evidence of great volcanic action, so do the Borrowdale series, and not the Welsh Lower Cambrians. One point for the former. The general correspondence of their strike with that of the Borrowdale series under Ingleborough may also perhaps count for something.

T. G. BONNEY

St. John's College, Cambridge, November 25

THOUGH the discussion of the age of the rocks of Charnwood Forest is not likely in the present state of our knowledge to lead to any useful result, there are still a few points in Prof. Hull's letter on the subject which seem to call for remark. In the first place the late Prof. Jukes was by no means so strongly in favour of the Cambrian age of these rocks as Prof. Hull states. Prof. Jukes' words, in Potter's (not Porter's) "History of Charnwood Forest" are as follows:—"It is therefore uncertain whether they (the rocks of Charnwood) belong to the Devonian, Silurian, or Cambrian systems, the probability only being in favour of the latter." Secondly, the Cambrian of Sedgwick includes a great deal more than the Cambrian of the Geological Survey, and therefore there is not the perfect unanimity between these two

authorities that Prof. Hull's remarks would lead us to believe. Thirdly, if lithological resemblance is to go for anything, it may be used directly against the Cambrian age of the rocks. On the western side of the forest we find sheets of crystalline rock and beds of highly altered conglomerates and breccias, which have a suggestive likeness to the lava flows and ash beds of the green slate and porphyry series of the Lake District. I don't say the resemblance proves anything, but it is worth quite as much as the similarity between the slates on the east side of the forest and the slates of Llanberis. Mr. Bonney has also called attention to the fact that the strike of the Charnwood Forest rocks is the same as that of the Volcanic Series in the Lake Country, when that group is last seen. Again, it is far from certain that the rocks of Charnwood Forest are all of the same age. I recollect seeing many years ago some sections (of which I am afraid I have kept no record) that seemed to show that some of the bosses of Dioritic rock near Markfield were older than the slates that surrounded them. If this be so, perhaps these crystalline hills may be the projecting points of a nucleus of similar rock that underlies the whole area, and which may be Laurentian in age. The rocks are not gneiss, but I know of no reason why the equivalents of the rocks of the Hebrides must be gneiss all the world over; they are, however, rich in hornblende, and so are the Hebridian rocks. With all these possibilities before us, I am afraid it will be hard to arrive at that enviable state of security which Prof. Hull seems to have been in when he penned his letter.

A. H. GREEN

I AM pleased to find in NATURE, vol. xv., p. 78, a letter from Prof. Hull, with reference to the age of our Charnwood Forest rocks. He writes against their assignment by Mr. H. B. Woodward, in his "Geology of England and Wales," to the Laurentian period (see p. 24).

But, in fact, as Prof. Hull himself points out, we also find on p. 30 a statement that part of the series may belong to the Cambrian epoch.

It would appear that as Mr. Woodward is not personally acquainted with the region, he has endeavoured to give the views of the various authors whom he knows to have written on the subject, and as these are conflicting, and based upon little personal work, it is no wonder that he has been led astray.

I do not think sufficient importance has been attached to the study of this isolated outcrop of old rocks. We can trace its continuation to the south and south-east for a considerable distance, and I would venture to suggest the possibility of a flexure or spur in this direction connecting with the old palæozoic ridge for which we have lately been fishing in the Wealden. In my "Geology of Leicestershire and Rutland," which will shortly be published, there will be found some fine photographs of the principal quarries and natural outcrops of the Charnwood rocks; and I have there given the reasons which induce me on the whole to refer the main mass of the rocks to neither Laurentian nor Cambrian, but to the *Silurian* period. The evidence is but scanty however, but a balance of probabilities at the best. As to Sedgwick's determination of the region, we must remember that much that he then called Cambrian has since been assigned to Lower Silurian.

WM. JEROME HARRISON

Town Museum, Leicester, November 24

### Minimum Thermometers

SOME time ago a correspondence appeared in NATURE (vol. vi. pp. 122, 142, 221) on the subject of moisture deposited in minimum thermometers exposed on the grass. As I was at the time much annoyed with this myself I took up every hint I could get in the matter, though I must confess with indifferent success. I tried for a long time india-rubber packing, with sealing-wax, &c., of varying coats, as advised, but still moisture insinuated itself.

At last I bethought myself of a cork packing. I cut a piece of cork so as to fit tightly round the neck of the thermometer tube, then inserted the tube and packing into the glass case—the cork packing being about a quarter of an inch long. The exposed end of the cork I covered with two or three coats of asphalt, as used on microscopic slides. At first a slight bubbling was seen through the asphalt, but soon disappeared, and a fine uniform surface at last set in. The thermometer has now been in use for several months, and not the least trace of moisture has ever been seen within the cases, although moisture has been abundant, especially for the last three months. The process is simple enough, and I venture to send it to you, hoping that it